

XXIII. *A Second Paper on Hygrometry.*By J. A. De Luc, *Esq.* F. R. S.

Read July 14, 1791.

P A R T II.

IN the first *part* of this Paper * I have treated of the fundamental principles of hygrometry, and of some hygroscopic phenomena; and this will relate to a particular application of those premises.

62. Since the publication of my first hygrometer, many others have been invented, two of which are now principally in use; the *hair* hygrometer of M. DE SAUSSURE, and my *hygrometer* made of a *slip of whalebone*. If the comparative points of those instruments could be determined in the whole extent of their scales, the only inconvenience of their being both used would be, the necessity of reducing to one of them, the observations made with the other; but from 70 to 100 of mine, which space includes the most important period of *moisture*, their correspondent indications are as different from one another, and as variable, as if they were the effects of two very different causes. Therefore it is important to decide which of them should remain our only measure of *moisture*, till, if possible, a better one is found. The following pages, I hope, will lead to that decision.

* See Page 1. of this Volume.

63. The fundamental process of M. DE SAUSSURE, with the view of discovering the effects of *moisture* on the *hair hygrometer*, was this. He repeatedly caused successive known quantities of *water* to evaporate into a close glass vessel, previously reduced to *extreme dryness*, and containing that *hygrometer* and a *manometer*; he observed the correspondent changes of those instruments, and, by combining the results of his experiments, he reduced to regular *series* the correspondent motions of the two instruments by equal quantities of *evaporated water*. Having confined himself to that only class of experiments, which, from causes that I shall explain, could not discover to him the difficulties of his attempt, he thought himself warranted to draw from them the following conclusions. 1st, That the degrees of *moisture* in the inclosed *medium*, were nearly proportional to the quantities of *water* evaporated in the vessel; and that, consequently, the *ratio* observed between those quantities and the *march* of his *hygrometer*, could be considered as giving immediately the *march* of the instrument correspondent to *moisture* itself; which, according to our common opinion, is a certain quantity of *aqueous vapours* spread in the *medium*. 2dly, That when no more *water* could *evaporate* in the vessel, the inclosed *medium* was arrived at *extreme moisture*; and that, consequently, the point indicated at that time on his *hygrometer*, was to be the *limit* of its *scale* on that side. 3dly, That having, from those experiments, a probable determination of the *expansions* of the *hair* by successive equal quantities of *moisture*, in beginning from the point where this is null, and ending at its *extreme*, his instrument could not differ essentially from an absolute *hygrometer*.

64. These conclusions were very natural in the state of M. DE SAUSSURE's experiments; but before their publication I had
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gone over a great field of *hygroscopic* phænomena, in which the *hair*, and a *close vessel*, had a share; and thereby, seeing the objects in another light than M. DE SAUSSURE, I doubted of his conclusions, and I procured three of his *hygrometers*, in order to examine them on some particular points. It was after that immediate verification of my conjectures concerning his instrument, that I settled the following conclusions, very different from those above. 1st, That *MOISTURE*, or the quantity of *vapour* spread in the *medium* itself, does not increase in an inclosed space in proportion to the quantity of water evaporated in it; because of an increasing, but undetermined, part of that *water* being deposited on the sides of the vessel; and that, consequently, Mr. DE SAUSSURE's experiments could not afford the determination of a real *hygroscopic-scale*. 2dly, That the circumstance considered by him as a sure sign of *extreme moisture* existing in the inclosed *medium*, namely, the *maximum* of *evaporation* in the space, has only that effect when the *temperature* is very little above 32° ; but that, by successive increases of *heat* from that point, *moisture* recedes farther and farther from its *extreme*; or from the point where no more *vapour* can be introduced in the *medium* without an immediate *precipitation*; though at the same time, there are successive increases in the quantity of *vapour*, and thereby a constant *maximum* of *evaporation* correspondent with the actual *temperature*. 3dly, That, in approaching to *extreme moisture*, the *hair* hygrometer becomes *stationary*, and afterwards a little *retrograde*, in which *march* the unavoidable irregularities of every *hygroscopic* substance produce frequent anomalies; from which cause it was very difficult for M. DE SAUSSURE, considering the form of his experiments, to discover the *hygroscopic law* expressed by the second conclusion; and with the unknown existence of that

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law, to suspect the *march* of his *hygrometer*: which accidental complication I shall explain hereafter.

65. When I published those results of my experiments and observations, M. DE SAUSSURE rejected them; not from having made new experiments that had confirmed his opinions; but because he conjectured inversely, that my theory resulted from a fallacious *march* of my *hygrometer*: and the well-earned reputation of that celebrated philosopher engaged me to undertake every experiment that could help me to detect on which side was the error. I have related, in the first *part* of this Paper, some of those experiments; and now, for their application, as well as for giving an account of some others, I shall follow more particularly M. DE SAUSSURE's process.

66. In a large glass vessel, containing (as I have mentioned above) a *manometer* and his *hair-hygrometer*, which vessel he had previously reduced to a known small distance from *extreme dryness*, M. DE SAUSSURE introduced from time to time a piece of *wet* cloth, which he weighed both before he put it into the vessel, and when he took it out. The successive increases in the quantity of *vapour* resulting from that process were indicated; on the *manometer*, by successive increases in the quantity of the inclosed *elastic fluids*, which caused the quicksilver to ascend more and more in that instrument; and on the *hygrometer*, by successive expansions of the *hair*. The *maximum* of *evaporation* was clearly indicated by the *manometer*; for, during every lasting *temperature*, the quicksilver, after having ascended to a certain point, remained fixed at that point, notwithstanding a longer stay of the *wet* cloth; and by repeating that operation at different *temperatures*, M. DE SAUSSURE determined the quantities of *evaporated water* that, in a given space, and by a given *temperature*, produced the *maximum* of *evaporation*.

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That single determination, from its great consequences not yet generally attended to, would be sufficient to fix the celebrity of its author, as I have already expressed many times in other works.

67. But with respect to a different conclusion from the same experiments, no less important to natural philosophy, I have also said, that M. DE SAUSSURE's *hygrometer* may have misled him. We have seen, that the *manometer* indicated, by an indubitable symptom, the *maximum* of *evaporation*; for here the immediate cause is clearly known, namely, the *quantity* of *elastic fluids*; and it is evident, that the quicksilver must ascend in the instrument in proportion to that *quantity*, and stop when it ceases to increase. But it is not the same as to the indications of the *hygrometer* in respect of *moisture*: M. DE SAUSSURE found himself that they were far from proportional to the intensities of their cause; and in the last stage of his experiments, though these indications did not vary much by the different *maxima* of *evaporation*, they however varied in the space of 1 or 2 *degrees*. But as those small differences on the point where the *hygrometer* stopped in different experiments, did not follow apparently any law conformable to the *temperature*, M. DE SAUSSURE considered them as small *anomalies*, unavoidable in hygroscopic substances, and of little consequence on a scale of 100 *degrees*; therefore, laying aside that circumstance, he could have no doubt, that, in every *temperature*, the *maximum* of *evaporation* in a close space was synonymous with the *maximum* of *moisture* in that space; while, from my experiments, these two supposed identical expressions may differ $\frac{1}{3}$, and sometimes $\frac{1}{4}$, of the real scale of *moisture*, which is the case in the *temperature* of only 75° or 80°.

68. Let us now suppose for a moment, that the above *hygroscopic law*, and the *march* that I attribute to the *hair hygrometer*, are real. In that case, if, during a constant *maximum of evaporation*, the *temperature* varies from 32° to 80° , *moisture* will diminish $\frac{1}{5}$ and even $\frac{1}{4}$ of the whole; or, in other words, the state of the *medium* will be distant by so much from that in which a new introduction of *vapour* would be followed by a *precipitation*. But at the same time, in the whole of that period of *moisture*, the *hair hygrometer* is supposed to move only 1 or 2 *degrees* backwards and forwards, with frequent irregularities. Therefore, in the hypothesis, such a great change of *moisture* would be hardly suspected from those small deviations of the *hair hygrometer*, in which at first nothing appears to be regular; and thereby it is evident, that, by confining himself to those experiments, M. DE SAUSSURE could not discover those two important laws of *hygrology* and *hygrometry*, of which I have here only supposed the existence.

69. Let us suppose again, that the *hair hygrometer* had not existed before a certain number of other experiments; and that M. DE SAUSSURE, in his attempt to produce an instrument of that kind, had fixed on any of the *slips* made of *fibrous vegetable* or animal substances cut *across the fibres*, of which many *hygroscopes* had been made before in a coarser manner; and that, in every other respect, he had proceeded as he has done with the *hair*. In that case, having placed his *hygrometer* and the *manometer* in the same vessel, with a quantity of *water* sufficient for producing the *maximum of evaporation* in every common *temperature*, and observed also the points where both instruments stopped in different lasting *temperatures*, he then would have found; that the *hygrometer* indicated less and less *moisture*, at the same time that the *manometer*, by ascending
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more and more as the *evaporation* increased by more *heat*, indicated that increase in the quantity of *vapour*, distinct from the expansions of the *fluids*; and that each of those instruments remained also fixed in that state of opposite changes, by every sufficiently durable change of *temperature*. The first observation of that phænomenon would have surprized him, as it did me, and it would have induced him also to try some other of those *slips*; and by finding the same phænomenon with every one of them, he would have been convinced, that it was a real *law* of *moisture*. Lastly, if he had known Mr. JAMES WATT's observations on the great *dryness* of the *steam* of *boiling water*, as long as it remains in a space as *warm* as the *water* that produces it (a condition always understood when the general laws of *evaporation* are the object of inquiry); admitting with me, as it follows from his own theory, that such *steam* is no other than the same kind of *vapour* thus far mentioned, only rendered capable of a greater *density* by more *heat*; he would then have seen the importance of that *hygroscopic law* from its great extent. I have hardly any doubt, that *extreme dryness* would reign in a close place similar to PAPIN's *digestor*, if there were a sufficient space above the *water* and a *red heat*; though that space would be filled with *vapour* to the *maximum*. This relates only to the *hygroscopic law* here in view, on which, in the supposed case, I do not think we could have had any controversy.

70. Lastly, let us suppose, that in order to try the effect of *moisture* on the substance he had first chosen, or on some other of the same kind of substances, but taken *lengthwise*, M. DE SAUSSURE had happened to try first one of those which, used in that manner, have a great *retrogradation*, as *goose-quill* and *deal*; and that, after having previously observed it in the

open air, he had inclosed it in the *moist vessel*, at a time when the place where it stood before had the degree of *moisture* corresponding with the *stationary* state of that *hygroscope*, he would then have observed a phænomenon as little expected as the former : for after that instrument had been inclosed in the *moist vessel*, it would have moved, by that increase of *moisture*, in the same direction as it had done in the open *dry* air when there was on the contrary an increase of *dryness*. Surprised no doubt at that phænomenon, M. DE SAUSSURE would have submitted his new instrument to more experiments ; he would also have tried other *threads*, in which he would have found the same sort of *march*, only at various degrees ; and if, in the course of those trials, he had submitted the *hair* to the same experiments, the smallness of its motions backwards and forwards, and their irregularities, would not have prevented him from discovering in it the same sort of *march* as he had then been used to see in other *threads* ; and thereby, he would have abandoned the whole tribe of *threads* as unfit for the *hygrometer*.

71. The whole of that supposed course of experiments with *slips* and *threads*, is that which I have followed from the time I had abandoned the construction of my first *hygrometer* ; which I did especially with the view of being able to try many substances. Therefore my theory was formed in consequence of the two above conclusions, which appear to me immediate, and such as M. DE SAUSSURE could not have drawn differently, if he had followed the same necessary steps : and now I will prove, moreover, that if it had not been for *accidental* circumstances in his own process, the *hair* alone would have engaged him by degrees to undertake the same experiments.

72. M. DE SAUSSURE's first *hygrometers*, having their *index* at the top of the frame, could be plunged into *water* ; and he
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tried that method for fixing their point of *extreme moisture*, as I had done for my first *hygrometers*. But in those trials he observed, that while his instruments stood in *water*, their indications remained undetermined within a space of four or five degrees; and attributing that irregularity to a *friction* of the *hair* with *water*, he thought it necessary to change my method, to that of placing the *instrument* in a *moist medium*, which he produced by means of a glass jar, *wet* on the inside, and inverted over *water*. In this method, the situation of the *index* became indifferent; and, for some particular reason, he placed it at the bottom of his new *hygrometers*, which then could not be plunged into *water*. This last circumstance was merely accidental; however, we shall see how much it has influenced his opinions in respect of *extreme moisture*.

73. In the first account of his experiments, M. DE SAUSSURE did not enter into those particulars; but they are in his answer to me: and when I there saw, that he gave as a reason for having *abandoned the immersion in water*, that a *strong adhesion* of the *hair* to that liquid impeded the freedom of its motions, I found it more natural to assign to the instrument itself the unsteadiness of its *index* which he attributed to that cause. Indeed, in those first *hygrometers*, one end of the *axis* passed through a hole for the purpose of carrying the *index* outwards, which was a cause of much friction: that *axis* besides was loaded with the weight of pretty large pincers, holding the *hair*, and that weight was counterpoised on the other side. Lastly, the connexion of the *hair* with the *index* was produced by a *silver lamina*, which, though very thin, opposed some resistance in bending round the *axis*. Those are defects that M. DE SAUSSURE corrected afterwards; but they existed in the instruments which he plunged into *water*, and a weight of

only 3 *grains* was not sufficient to keep their *index* steady, either in *water* or any where; and this he observed himself.

74. Notwithstanding that natural explanation of the unsteadiness of M. DE SAUSSURE's first *hygrometer* when in *water*, I thought it necessary to try, in the same circumstance, some well-constructed *hair hygrometer*; therefore I made two, similar to the last of M. DE SAUSSURE's in every respect, except that of having their *index* at the top; and for the connection of the *hair* with the *axis*, I used a kind of *pincers*, not above half a *grain* in weight, with a hair-like bit of *bemp*, which I know does not alter in any sensible manner the march of the instrument. Now, these *hair hygrometers*, with their *weight* of only 3 *grains*, being put into *water*, follow in it their own laws, arrive and remain *fixed* at their once settled point, as well as any of my other hygrometers. This M. DE SAUSSURE would have also found, if in the improvement of his instrument the *axis* had remained at the top.

75. Before I explain the influence which that *accidental* circumstance of the place of his *index* has had on his opinions with respect to *extreme moisture*, I must mention another of the same kind which has contributed to the same effect. It is evident, both from theory and from M. DE SAUSSURE's own experiments related above (§ 66.), that a sufficient quantity of *water* in any part of a close vessel is the only requisite for producing in it the *maximum* of *evaporation*; but, with a view of accelerating that effect for the common purpose of fixing the point of *extreme moisture* on his *hygrometer*, he prescribes *wetting* the inside of the vessel, besides inverting it over *water*. He did not (and indeed he could not) foresee the consequence of that alteration in his first process; but in fact it was such as to prevent him from discovering, even with time,

time, in those operations, what he would have seen immediately if his last *hygrometers* could have been plunged into *water*.

76. After the improvement of his *hygrometers*, their *index* was no more subject to that unsteadiness observed in the first; consequently they remained *fixed* under the *moist vessel*; but they did not *fix* at the same point every time; and it happened *accidentally*, from causes of irregularity in the *vessel* itself, that in some of the cases, when the *hair* was the *longest*, a *precipitation* of *water* happened (by some partial cooling) on some part of that vessel; and he took that appearance for a sure sign that there was a *superfluous* quantity of *water* in the inclosed air. From that *accidental* connexion of circumstances he concluded, that his *hygrometer* indicated two different states of the *medium* in respect of *extreme moisture*; one, correspondent to about 98 on that instrument, which he considered as real *extreme moisture*, or that state of the *medium* in which no more *vapour* could be introduced into it without a *precipitation*; the other, when such a *precipitation* took place, which he made correspondent to 100, or to the greatest *length* of the *hair*. Prepossessed afterwards with that opinion, when I published my comparative experiments of his *hygrometer* and mine, in which the latter stood sometimes at 80 when the former was at 98, he concluded from that circumstance, that while his *hygrometer* moved only 2 *degrees* by the utmost effect of a *super-saturation* of the *medium*, mine had 20 of those *insignificant degrees*. In this centers the whole of our disagreement, and I am now going to trace its cause in the *accidental* circumstances above described.

77. When I plunge my *hair hygrometers* into *water*, where, as I have said, they come to a *fixed* point, that point does not indicate the *greatest length* of the *hair*; for, on the contrary,
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that *thread* is then *shorter* than it is most times under the *moist vessel*. This M. DE SAUSSURE would have seen, if he had not been prevented, by the situation of the *index* in his improved *hygrometer*, from trying again the effect of *water* on the *hair*; and that phenomenon alone would surely have given a different course to his ideas; especially he would not have supposed, that the *hair* lengthens 2 degrees more, by a *super-saturation* of the *medium*, or by the immediate contact of concrete *water*.

78. If, also, when he settled the manner of determining the point of *extreme moisture* on his *hygrometers*, M. DE SAUSSURE had retained the simplicity of the process he had used for his fundamental experiments, in which a piece of *wet* cloth had been sufficient for producing the *maximum* of *evaporation* in his large vessel; and, in consequence, had contented himself with inverting his glass jar over *water*, without *wetting* it on the inside, he would have avoided a great cause of deception which I am going to explain. In my first experiments on the comparative *marches* of our *hygrometers*, in which I followed M. DE SAUSSURE's prescription for the *moist vessel*, I found some *anomalies* which puzzled me. M. DE SAUSSURE himself took notice of them in the account I gave of those experiments, and attributed them to my instrument. I did not agree with him in that respect; but it was long before I could discover the real cause of those anomalies. The first step towards that discovery, was the reflecting on the uselessness of *wetting* the vessel on the inside, for the only purpose of producing in it the *maximum* of *evaporation*. That consideration engaged me to undertake a new course of the same experiments, with a glass jar merely inverted over *water*; and by that means, the greatest part of the real *anomalies* being removed, I observed clearly in the *march* of the *hair*, the combined

combined effects of its own property, and of the *hygroscopic law* respecting *evaporation* which was known to me from other phænomena.

79. As for the cause of those *anomalies* which had been removed by the change of the process, the following phænomenon led me to discover it. I observed frequently, at times when my *hygrometer*, placed under the jar, stood at a considerable distance from its point of *extreme moisture*, that a very small diminution of heat was sufficient to cause, on the lower part of the vessel, the formation of a *tarnished rim*, extending one or two inches above the surface of the *water*, with a thin vanishing edge. Having reflected on that phænomenon, from the mechanism I assign to the operation of *fire* in the very act of *evaporation* I concluded, that in such a *stagnant air* every *evaporating* surface had an *atmosphere* of *extreme moisture*, which extended as indicated by the *tarnished rim*; and that it was only beyond that limit that reigned the other *law*, of a decreasing *moisture*, correspondent to the increasing *maxima* of *evaporation* by an increasing *heat*. That new *law* of *evaporation* offered evidently an adequate cause for explaining the anomalies observed in the *wet* vessel; for *glass* retains concrete *water* very imperfectly, and it runs down very soon from many of the places which had retained it. Consequently, under such a partially *wet* vessel, and differently so at different times, the instruments must be variously affected by scattered *atmospheres* of *extreme moisture*.

80. However, before I could trust that explanation, I wanted to submit it to some direct experiment; and I succeeded by a means which at the same time realized what M. DE SAUSSURE thought he had obtained, namely, to produce *extreme moisture* in a close space, during any common *temperature*,

without any *precipitation* of *water* from the *medium*. This I have produced by means of a wire cage, 4 inches in diameter, covered with cotton cloth, having at the top a *reservoir*, by which the cloth is kept thoroughly *wet* for a long time; which cage besides is inclosed in the glass jar inverted over water. In that apparatus, though in summer-time, every *hygrometer*, either *thread* or *slip*, moves and fixes itself, not so speedily, but else exactly as if it was plunged into *water*, without any *super-saturation* of the inclosed *medium*, or *precipitation* of *water* on the *hygroscopic* substance.

81. We may see now that the idea of two sorts of *extreme moisture* is without any foundation. In order to enforce the necessity of taking the point of *extreme moisture* in the *air* brought to that state, and not in *water*, M. DE SAUSSURE says, "That the *hygrometer* is not to measure the *moisture* of *water*, " but that of the *air*." This at first appears plausible; however, in reality, *moisture* is no more to be considered in *water* itself, than *heat* in the fluid called *fire*. *Water* is the cause of *moisture*, as *fire* is the cause of *heat*; but those *effects* are not produced *on* their causes; it is *on* other substances. Therefore, if some *hygroscopic* substances are placed in a *medium* which has attained *extreme moisture*, and, in proportion as they take *water* from it, the loss of that *water* be constantly repaired by a new *evaporation*, they will receive by degrees in such a *medium*, without any *precipitation*, as much *water* as if they were plunged in *water* itself; for the limit is their *capacity*, which I have explained in § 19. This is the same theory that I had expressed in my first Paper on *Hygrometry*; and it is completely confirmed by the above experiment, with the discovery of this new *hygroscopic law*: "That in a *stagnant air*, every " *evaporating* surface has an *atmosphere* of *extreme moisture*,
" which

“ which extends in a space of few inches, diminishes rapidly,
 “ and does not interfere beyond that limit with the other *laws*
 “ of *moisture*.”

82. I shall now explain, by an example, what is the fundamental *deviation* of an *hair hygrometer*, introducing in it, for a moment, those two singular points 98 and 100, which, in M. DE SAUSSURE's experiments, were a very natural cause of mistake. A *hair hygrometer* and mine, being in a close vessel, at a time when the *temperature*, sensibly constant, shall be but little above 32; if *moisture* is first introduced into that vessel, so as to bring the *hair hygrometer*, by a very flow direct motion, to 98, my *hygrometer* will stop between 70 and 75; and both instruments will be *fixed*, if *moisture* and *heat* remain the same. Let *moisture* then be made to increase very slowly, till the *hair hygrometer* has attained its point 100; mine will have arrived at 80; and they again will remain at those points as long as, with the same *temperature*, the same quantity of *vapour* shall remain in the vessel. Lastly, let a sufficient or superfluous quantity of *water* be introduced into the vessel, the *hair hygrometer* will *retrograde* to 98, and mine proceed to 100, at which points they will stop, whatever be the quantity of *water*; and they will remain fixed, as long as the *heat* shall not increase. This explains the riddle of the singular point 98, or of a certain *point*, various in different *hair hygrometers*, various even, at different times, in the same individual, at which that instrument stands with very different degrees of *moisture*; consequently, its little motions round that *point* may create great deception, especially from the nature of *organized* substances, on which now I shall add a few words.

83. The above is the fundamental *march* of the *hair hygrometer*, such as it is on the whole, and as it would be con-

stantly, if no other cause interfered ; but it is subject to disturbing *anomalies*, which become worthy of attention within that small critical space which I have described. The *texture* of organized *hygroscopic* substances occasions a *friction* between their parts, when, by the changes of *moisture* and *heat* combined with their *elasticity*, they undergo changes in their respective positions ; whereby they hardly can completely return to the very same arrangement, though with the same external circumstances ; even at *extreme moisture*, when, there being less *friction* between their particles, the greatest part of the disturbances produced in their former motions are restored. To that general cause of irregularity is added a particular cause, when those substances are in the state of *hygrometers* ; this is the influence of two opposite forces acting constantly on them ; one, the tendency of their component parts to remain united ; the other, a *weight* or *spring* which tends to separate them. Certain accidental arrangements of their component parts give them more power to resist the action which tends to separate them ; and those arrangements are very changeable, by the alternate introduction and expulsion of *moisture*, by a long stay within a small compass of variations, and by more or less *heat*. This is a large field of facts and speculations, not uninteresting in itself, but on which I must not dwell : what I have said of those causes is sufficient to account for the *anomalies* to which, more or less, every *hygrometer* is subject. But whereas in the *slips*, those *anomalies* create only some irregularities in the observations, without any deceiving consequence in respect of the *laws* of *moisture*, they may deceive when they happen to interfere in the critical part of the march of some *threads* ; for instance, if, by a certain accidental arrangement of the constituent parts in a *slip* of *whalebone*, there happens to be some

tenths of a *degree* difference, from one experiment to another, on its point of *extreme moisture*, that *anomaly* cannot be of any consequence on the determination of what must be considered as that state in the medium; but if it happens to the *hair*, which, in approaching *extreme moisture*, has but very small motions, it may reverse those which it had had naturally (as I have observed it sometimes) and become a cause of deception.

84. I have now explained, how mere *accidental* circumstances have been the cause of a difference in the ideas that M. DE SAUSSURE and I had formed on what is to be understood by *extreme moisture* in every case; and I am going to illustrate the whole of that subject by a singular fact. An *hygrometer* made with a *box thread*, or a thin *fascicle* of the fibres of that wood, being placed in open air, next to a *hair hygrometer*, or to most of the other instruments of that sort, moves in a contrary way from them; but we may lay aside that circumstance, by supposing, that the *numbers*, marked on the *dial* of the first, are increasing in the opposite direction from those of the other instruments. Let us then suppose, that some experimental philosopher had chosen the *box thread* for his *hygrometer*; with him I should have fallen into no controversy on the point of *extreme moisture*; for, either under the *moist vessel*, or in any other case approaching *extreme moisture*, his *hygrometer* would have moved like mine. But the *box thread*, at approaching *extreme dryness*, first relents much its pace, then becomes *stationary*, and afterwards *retrograde*; by which property, with the concurrence of some *accidental* circumstances as have happened in M. DE SAUSSURE's experiments, the very same questions that I have examined with so much labour in respect of *extreme moisture*, only because of the *hair hygrometer*, would have been transported to the point of *extreme*

treme dryness; which, however, till now, has created no doubt.

§5. That singular *thread* illustrates also another point, closely connected with the *retrogradation* of its tribe, but not to be confounded with it; I mean the *recoil*. The common cause of both phænomena is, two opposite *effects* produced by changes of *moisture*, on the length of *threads*. The *retrogradation* in the *march*, comparatively to that of *moisture*, is produced by one of the *effects*, which before was surpassed by the other, becoming predominant; and the *recoil*, or a returning back a part of a first stride when *moisture* changes suddenly, is produced, by one of the *effects*, that on the *fibres* themselves, being performed *sooner* than that on the sort of *reticle* formed by the *fibres*. Now, the *box thread* having its *retrogradation* at approaching *extreme dryness*, there also the *recoil* becomes sensible: it appears in the first modifications of that *thread* when placed in my *dry vessel*, by motions backwards and forwards, as it happens to some other *threads*, when taken out of *water*, or exposed to any other sudden change of *moisture*. The *box thread* having a slow motion, no *recoil* is clearly distinguishable in its common *march*; whereas the *hair* and the *quill-thread*, which, in appearance, are very quick, have generally a very disturbed motion when *moisture* changes suddenly. I have seen them, when in a free air, and happening to be in their *stationary* state, moving quickly one way in a space of 1 or 2 degrees, and then *recoiling* slower, sometimes to the same point where they were before, while my *hygrometer* underwent a steady change, which was in the first direction of theirs. This phænomenon, of a complete *recoil* in the *stationary* state of quick *threads*, is similar to the *recoil* of the *index* in those *glass frames* that I have described in § 59. of this Paper, which have a

compensation for the changes of *beat* by a thin *brass lamina*: this being sooner affected than the *glass rods*, by sudden changes of *beat*, the *index* moves first one way; then it *recoils* completely, by the change being operated later in the *glass rods*.

86. The experiments briefly related in this Paper will, I hope, be sufficient to answer the following question, which has been made to me by some observers of the two principal *hygrometers* here compared: “Why does the *hair hygrometer*, “when exposed to the open air in day time, come so often “near its point of *extreme moisture*, while the *whalebone* hygrometer almost never comes within 30 *degrees* distance of that “point in summer, and very seldom within 20 in winter, even “in *rainy* weather (if preserved from rain)?” The answer, according to the results of these experiments, is this: “The “general *march* of the *hair hygrometer* is much *decreasing*, “comparatively to equal increases of *moisture*; that *march* for- “wards, ends in a *stationary* state, and is followed by a small “*retrogradation*; while the *whalebone* hygrometer has con- “stantly a *march*, if not *proportional*, at least constantly *similar* “to that of *moisture* itself.”

87. There remains an object of inquiry, which is, a determination of those *ratios* here generally expressed. I have explained, in the first *part* of this Paper, the difficulties of that object, and what help may be found in comparing the *marches* of *hygrometers* with the *acquisitions* of *weight* of their substances, of which process I gave some *examples*; and here I shall relate similar experiments on *hair*, *whalebone*, *box*, and *aloes-pitta*. But as I have already described the whole process, and the manner of calculation of that class of experiments, I shall only give here the results of these last.

88. I must, however, first explain another *reduction* that I have added to the former. In the first TABLE which I have given of those experiments, I followed the immediate division of my instruments, in which 0 corresponds to *extreme dryness*, and 100 to *extreme moisture*. But first, under that form, the point called 100 by M. DE SAUSSURE would not appear in its true light, as it means the greatest *length* of the *hair*; while the point 100 of my scale designs the state of that *thread* in *water*, where it has a *little retrogradation*. Under that form also most part of the terms in the observation on the *box thread* would be *negative*, since it moves very long in a contrary direction to the other hygrosopes. For these reasons, instead of calling 0 the point of *extreme dryness*, and that of *extreme moisture* 100, I have, in the following TABLES, applied the first of those denominations to the *smallest length* of each substance, and the last to their *greatest length*. That *reduction* produces no difference in the *proportions* between the *terms*, and none in the *terms* themselves in respect to *slips*; as in these, the *greatest length* is always observed by *extreme moisture*, and the *smallest* by *extreme dryness*.

II. TABLE

II. TABLE of comparative changes in the weight and in the length of the same substances, by the same increases of moisture, correspondent to the march of the slip of whalebone from 5 to 5 of its degrees.

	WHALEBONE.		HAIR.		ALOES-PITTA.	
	Increases of the weight in shavings.	March of the slip.	Increases of the weight in a mass of hair.	March of the hair.	Increases of the weight in a mass of pitta.	March of the thread of pitta.
<i>Extr. dryness</i>	0.0	0	0.0	0.0	0.0	0.0
	6.0	5	4.8	15.7	6.0	20.6
	11.8	10	8.8	29.0	11.8	35.1
	17.3	15	12.5	40.0	17.3	51.6
	22.2	20	15.9	50.4	22.2	57.6
	26.8	25	19.1	59.7	26.8	75.6
	31.2	30	22.2	67.5	31.2	71.9
	35.2	35	26.6	74.4	35.2	76.3
	39.7	40	29.0	79.3	39.7	83.0
	44.0	45	32.0	83.3	44.0	86.6
	48.1	50	35.0	88.0	48.1	93.6
	52.1	55	38.2	90.0	52.1	96.5
	57.1	60	43.3	92.8	57.1	94.7
	61.7	65	49.8	94.1	61.7	98.2
	66.3	70	55.3	95.4	66.3	100.0
	71.9	75	61.9	97.0	71.9	99.2
	77.6	80	68.7	100.0	77.6	98.2
	* 83.2	85	* 76.0	99.5	* 83.2	96.8
	* 88.8	90	* 84.0	99.2	* 88.8	94.1
	* 94.4	95	* 92.0	98.6	* 94.4	91.9
<i>In water</i>	* 100.0	100	* 100.0	97.7	* 100.0	88.3

89. In the above, and in the following experiments, the operation of successively introducing *moisture* into the vessel was stopped, when the *slip* of *whalebone* was at 80, as beyond that term the smallest difference in the *temperature* between the parts of the apparatus creates great anomalies; therefore the fol-

lowing *terms* in the three columns of *weights*, which are marked with an *, have only been added (as I have explained for the former *Table*) with the view of having a common *modulum* between the changes of *weight* and the *marches* of the other instruments. But the observed *terms* remain in their original *proportions*, and from these we may see, that the *march* of the *slip* of *whalebone* does not differ much from the successive increase of *weight* in its own substance; and that when taken out of the apparatus, and immediately plunged into *water*, it proceeds in the same direction as before, till it has attained its fixed point, while the *hair* takes, comparatively with the increase of *weight* of its own substance, great strides in the beginning of its *march*, and very small steps in the latter part of it, before 100; and then *retrogrades* a little, when taken out of the apparatus, and plunged into *water*. We see besides in that *TABLE*, that the *thread* of *aloes-pitta*, which at first takes still greater strides than the *hair*, has, after a longer *stationary* or undetermined state, a determined beginning of *retrogradation* at the same time that its own substance continues to acquire *weight* in the apparatus, and continues that *retrograde* march when, being taken out of the vessel, it is plunged into *water*.

90. The following *TABLE* will farther illustrate these characteristic differences of *slips* and *threads*, the ascertaining of which was so essential to *hygrometry*.

III. TABLE

III. TABLE of experiments on the comparative changes in the weight and the length of the same substance by increase of moisture.

		B O X.		
	Slip of whalebone.	March of the slip.	Increases of the weight in shavings.	March of the thread.
<i>Entr. dryness</i>	0	0.0	0.0	72.8
	5	4.5	7.3	87.2
	10	9.5	12.8	93.2
	15	14.5	17.8	97.8
	20	20.0	22.6	100.0
	25	25.7	27.3	95.9
	30	31.5	31.8	92.7
	35	38.0	38.5	88.6
	40	45.5	44.5	79.9
	45	51.5	49.7	70.3
	50	56.5	54.8	63.9
	55	61.2	59.1	57.3
	60	65.7	63.1	51.0
	65	69.7	66.4	47.5
	70	73.7	69.6	40.9
	75	77.7	76.6	31.4
	80	81.5	80.0	21.7
	85	85.9	* 85.0	16.0
	90	90.5	* 90.0	10.4
	95	95.5	* 95.0	5.1
<i>In Water</i>	100	100.0	* 100.0	0.0

We see in this TABLE the *slip* of *box* following, in its increases of *length*, the increase of *weight* in the *shavings* of the same *wood*, nearly in the same manner as the *slips* of *whalebone*, *quill*, and *deal*, follow those of their own shavings; while the *thread* of *box*, after having gained some length by

decreasing steps, begins soon to *shorten*, at the same time that its substance continues to imbibe *water*; being thus the *shortest*, when it cannot receive any more *water* in its pores. That excess of the *hygroscopic* phenomenon of *threads* cannot but throw a full light on the nature of those *hygroscopes*.

91. I am now going to assemble, in two TABLES, the comparative *marches* of all the *threads*, and of all the *slips*, which I have hitherto submitted to that regular course of experiments, laying aside many more of each class, the *marches* of which I only know from common observations. The next TABLE shall contain the experiments on *threads*; in the number of which are two thin natural bodies, which in that respect are similar to the *hair*; one, an animal substance, is a very thin *porcupine quill*; the other, a vegetable, is a thin stem of *gramen*.

TABLE of the correspondent marches, by the same increases of moisture, of different THREADS or vegetable and animal substances taken lengthwise.

	Porcupine quill.	Whale-bone.	Hair.	Cut.	Aloes-pitta.	Goose-quill.	Deal.	Grass.	Box.	Slip of wh. bone.
<i>Ext. dryness</i>	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	72.8	0
18.0	12.0	15.6	9.7	20.6	37.0	33.2	26.8	87.4	5	
34.0	29.9	29.4	19.2	35.1	66.6	54.8	48.4	93.2	10	
48.8	39.9	40.9	26.8	51.6	78.7	74.9	67.1	97.8	15	
62.3	50.8	50.5	37.0	57.6	88.0	84.6	76.6	*100.0	20	
73.3	58.8	59.2	47.1	75.6	+ 93.4	89.8	83.9	95.9	25	
81.0	65.3	68.8	57.3	71.9	97.2	93.8	90.5	92.7	30	
86.8	70.8	73.0	67.4	76.3	99.0	96.9	95.1	88.6	35	
90.8	76.1	78.3	75.6	83.0	94.4	94.3	98.6	79.9	40	
93.0	81.4	82.1	82.9	+ 86.6	96.2	97.7	*100.0	+ 70.3	45	
95.0	85.4	86.1	87.8	93.6	99.0	*100.0	98.8	63.9	50	
94.5	88.4	88.8	+ 91.6	96.5	95.3	94.6	98.0	57.3	55	
97.0	90.8	91.6	94.7	94.7	97.2	97.0	97.2	51.0	60	
96.5	92.8	93.8	96.3	98.2	98.2	94.6	96.2	45.7	65	
96.5	95.1	95.6	97.8	*100.0	*100.0	93.0	94.8	40.9	70	
95.0	97.1	97.2	98.7	99.2	99.0	91.4	92.6	31.4	75	
97.0	98.1	+ 98.0	*100.0	98.2	98.2	89.0	89.8	21.7	80	
98.0	99.1	100.0	98.7	96.8	97.2	86.9	86.5	16.0	85	
98.6	+ 99.6	*100.0	96.8	94.1	95.8	84.6	84.0	10.4	90	
99.1	*100.0	99.3	94.5	91.5	94.4	81.9	80.9	5.1	95	
In water	100.0	99.5	98.3	91.8	88.3	92.5	79.0	77.0	0.0	100

92. There the *porcupine quill* shews no *retrogradation*; however, consistent with its tribe, it had some in other experiments. Its last steps have the unsteadiness of the *stationary* state, and thereby are subject to anomalies. From the same cause, none of the other *threads* have exactly the same steps in any two experiments, though on the whole their *march* remains essentially the same. The *march* here given of the *hair* hygrometer comparatively with mine, is the mean result of three experiments, with three different sets of instruments; one of the *hair* hygrometers that I have employed was sent to me by Mr. PAUL, of Geneva, and its point of *extreme moisture* was determined in a *fog*. The small and changeable *retrogradation* of the *thread* of *whalebone* and of *hair* might have been overlooked, were it not for other *threads* in which the *retrogradation* begins before that period where the state of *moisture* is difficult to ascertain; but from these *threads*, that phænomenon is placed in a clear light, which is reflected on the others. I have marked with an * the greatest *elongation* of each of them, and with a † a point near which their *elongation* begins, and to which they return at last. These signs will guide the eye in the above TABLE, which shews clearly, that no *thread* can be trusted to for the HYGROMETER.

TABLE of the correspondent marches of slips, or of fibrous vegetable and animal substances taken across the fibres, and of such as have no sensible fibres.

	Goode quill.	Porcupine quill.	Slip of wh. bone.	Box.	Deal.	Ivory.	Ivory.	Tortoise-shell.	Horn.	Horn.
					breadthwile.	lengthwile.	lengthwile.	breadthwile.	lengthwile.	lengthwile.
Ext. dynes	0.0	0.0	0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
4.8	4.8	4.8	5	4.5	5.4	6.1	8.3	11.0	9.5	13.8
9.7	8.8	10	9.5	11.2	12.7	16.6	21.5	18.5	26.8	38.8
14.4	12.0	15	14.5	16.5	18.7	24.6	31.5	27.5	38.8	48.8
19.2	17.0	20	20.0	21.9	24.9	31.5	38.5	37.0	48.8	58.0
23.9	23.4	25	25.7	27.2	30.4	37.6	45.4	46.5	58.0	64.6
28.5	29.4	30	31.5	32.7	35.4	43.6	51.9	54.5	64.6	71.0
33.3	36.0	35	38.0	38.3	41.9	49.7	58.3	62.1	71.0	75.5
38.3	41.4	40	45.5	43.7	47.4	56.3	63.8	68.7	75.5	78.5
42.9	45.4	45	51.4	49.2	53.5	62.4	69.0	72.3	78.5	82.2
47.4	49.8	50	56.5	54.6	58.5	67.4	72.8	77.0	82.2	86.2
52.4	54.8	55	61.2	59.9	63.5	71.6	76.4	79.7	86.2	89.6
56.9	59.7	60	65.7	64.9	68.0	76.1	79.4	84.3	89.6	92.4
61.9	64.4	65	69.7	69.7	72.1	79.1	82.4	86.4	92.4	93.4
67.2	68.5	70	73.7	74.5	76.1	82.9	84.9	88.4	93.4	94.4
72.2	73.5	75	77.7	79.0	80.1	86.7	88.2	90.2	94.4	95.4
77.8	78.9	80	81.5	83.5	84.5	90.4	91.2	92.0	95.4	96.6
82.8	83.9	85	85.9	87.5	87.8	92.4	93.8	94.0	96.6	97.8
88.2	88.9	90	90.5	92.0	92.0	94.5	96.2	96.1	97.8	99.0
94.0	94.4	95	95.3	96.0	96.0	97.5	98.6	98.1	99.0	100.0
In water	100.0	100.0	100	100.0	100.0	100.0	100.0	100.0	100.0	100.0

93. This last TABLE is the most important, as it contains a class of *hygroscopes* which possess in common the following first requisites for an *hygrometer*; 1st, that they may indicate, without any illusion, both *extreme dryness* and *extreme moisture*; 2dly, that they move constantly in the same direction as *moisture* itself; 3dly, that they move always when *moisture* changes. It should seem as if the *march* of the *slip* of *horn* taken *lengthwise*, from its very decreasing progression, came very near that of the thin *porcupine quill*; but, as I have said, among the steps of the latter there are accidental *retrogradations*, and it sometimes has a final one; and I have never observed that disposition in the former, which, in its last small steps, follows constantly the motions of every other *slip*.

94. The agreement of all the *slips* in this last respect is a very essential circumstance in hygrometry, as it assures us, that we cannot mistake the cases when *moisture* is *extreme* in the *atmosphere*; a very important point for discovering the nature of many *meteorological* phænomena. No *slip* will create deception in that respect; while, on the contrary, every *thread* may deceive in dubious cases, and even create great error, if, unknown to the observer, it happened to be in the beginning of its *elongation*. There was, however, a question to be decided in that respect, namely, whether or not a great *moisture* in the *medium* was a cause of alteration in the *march* of any *hygroscope*, by producing in its substance a sudden irregular *lengthening*. That accidental question is answered in the negative by all the *hygroscopes* of both classes: for, in respect of the *threads*, instead of *lengthening* suddenly in that period of *moisture*, they have then a *retrograde* motion, either continuing or only beginning; and as for the *slips*, they, by *lengthening* in the same period, only follow their former *laws*: the *slips* which

which, comparatively to that of *whalebone*, have at first small *steps*, and which consequently move in an increasing progression, continue only to follow that progression; and those which at first have greater *steps*, and consequently a decreasing march, have then small *steps* conformable to their individual *law*; therefore, none of those *hygroscopes* of both classes have any *sudden start*, produced by any degree of *moisture* in the *medium*, or by the application of concrete *water*; each of them follows, from one end to the other of its scale, its own *progression*; and in respect of *slips*, moisture is never *extreme* in the ambient *medium*, as long as, in their respective *progressions*, they have not attained their *greatest length*.

95. Our common *hygrometer* must then be made of one of the *slips*; but with that great dissimilarity observed in their *marches*, which of them shall we choose as indicating the real *march* of *moisture*? None as yet from that consideration, which I do not even think a primary one. It is true, that if we trust to the increases of *weight* in those substances, as being a means of ascertaining the real progress of *moisture* in the ambient *medium*, the mean rate of six experiments of that kind related in this Paper, give the preference to the *slip* of *whalebone*; but this I do not yet consider as decisive, farther than in what relates to the comparative *marches* of *slips* and *threads*; however, as my reasons of doubt on a more absolute conclusion cannot be expressed in a transitory manner, I must lay them aside for the present.

96. But, as I have said before, this is not what ought to determine our choice on the *substance* of a common *hygrometer*, since the observations themselves are distinct from the consequences to be drawn from them. Let us suppose the case (which I do not give up) that, with time and researches, some

process be found by which *known* quantities of *moisture* may be successively produced in the *medium* itself. The use of that process for hygrometry will be, as M. DE SAUSSURE has begun to do it, to observe, on some *hygrometer*, the successive effects of those known quantities of *moisture*, from which may be formed a *table* of the correspondence between the *equal degrees* of the scale of the chosen instrument, and the real quantities of *moisture* in the *medium*; and that *table* will serve to correct as well *past* as *future* observations made with that instrument. Therefore it matters not what that *hygrometer* shall be, provided it is convenient in other respects. Let us then examine which of the *slips* possesses the most essential properties of an *hygrometer*, such as should be in common use for comparative observations, and to which consequently future discoveries in respect of the real proportions between the quantities of *moisture* itself would be applied.

97. *Steadiness* is surely a first requisite for such an instrument; and in that respect no *slip* comes in competition with that of *whalebone*. That property was the first motive of my choice; and as an instance of it I shall only mention, that I have just now plunged into *water* an instrument of that sort, of above ten years standing, which is come to its point of *extreme moisture* as if it had been fixed yesterday; for, without regard to the distance of observations, there may be between them a difference of some *tenths* of a *degree*. Some other *slips* may be brought to a certain degree of *steadiness* by studying what is the degree of *stretch* which they may bear; but that attention is not necessary for the *slip* of *whalebone*: if, for instance, when its point of *extreme moisture* has been fixed while it was *stretched* to a certain degree, that *stretch* is

much

much increased, it will acquire some absolute *length*; but it will be *steady* again for a new point taken then in *water*.

98. Another property of the *slip* of *whalebone*, which at first should seem contradictory to the former, is its great *expansibility*, in which also it surpasses all the substances which I have tried. Such a *slip* lengthens above *one-eighth* of itself from *extreme dryness* to *extreme moisture*, which produces many advantages in the construction and observation of that instrument. In respect to observation, when it is exposed to the wind, the difference between the chords of the arches of its bends and its real *length* is so small, comparatively with its *hygroscopic* variations, that the indetermination of its *index* will remain confined in a space of one or two *degrees*, when it becomes impossible to observe *hygrometers* whose substance has but little *expansion*. Lastly, of all the substances which I have reduced to *slips*, none is so easily made thin and narrow as *whalebone*. I have found means for producing easily such *slips* of it as, with a length of eight *inches*, weigh only about $\frac{1}{16}$ th of a *grain*, and are thereby as *quick* as is convenient in other respects. All those distinctive properties of the *slip* of *whalebone* seem to point out an *hygroscopic* substance fit for our *common hygrometer*.

Description of the whalebone hygrometer.

99. I have now only to describe the construction of that instrument as I have fixed it after a long experience. The *fig. 1.* (Tab. IX.) shews its form for common use. Some of those instruments are of the same size as the *figure*, and they may easily be made smaller, but commonly they are half as large again in every dimension. Their frame will sufficiently be known from the *figure*,

therefore I shall confine myself to the description of some particulars. The *slip* of *whalebone* is represented by a, b ; and at its end a is seen a sort of *pincers*, made only of a flattened bent wire, tapering in the part that holds the *slip*, and pressed by a sliding ring. The end b is fixed to a moveable bar c , which is moved by a screw for adjusting at first the *index*. The end a of the *slip* is hooked to a thin brass wire; to the other end of which is also hooked a very thin silver gilt *lamina*, that has at that end *pincers* similar to those of the *slip*, and which is fixed by the other end to the *axis* by a pin in a proper hole. The *spring* d , by which the *slip* is stretched, is made of silver gilt wire; it acts on the *slip* as a *weight* of about 12 grains, and with this advantage over a *weight* (besides the avoiding some other inconveniencies of this) that, in proportion as the *slip* is weakened, in its lengthening by the penetration of moisture, the *spring*, by unbending at the same time, loses a part of its power. The *axis* has very small *pivots*, the *shoulders* of which are prevented from coming against the frame, by their ends being confined, though freely, between the flat bearing of the heads of two *screws*, the front one of which is seen near f . The section of that *axis*, of the size that belongs to a *slip* of about 8 inches, is represented in *fig. 2.*; the *slip* acts on the diameter a, a , and the *spring* on the smaller diameter b, b .

100. Another construction of the same instrument is represented, of half its dimensions, by *fig. 3.* (Tab. X.) The essential parts of that *bygrometer* are the same as in the other; therefore I shall only mention the differences adapted to the use of fixing it out of a window. The *figure* represents the manner in which it is fixed, with its *dial* turned half-way towards the observer, by a proper head of the hooks which hold it at the top and the bottom.

bottom. The steady part of the instrument is represented by *fig. 4.*; it is partly an half tube, cut longitudinally through the axis, and having sidewise two vertical rows of large holes. A whole tube goes over that half one; for which purpose the piece *a, a,* is taken off by unscrewing the female screw *b* which belongs to the part *d*; this last is a small open tube, cut outside in screw. The large external tube has also two vertical rows of holes, at such a distance from one another, that, when one of them corresponds to one of the rows of the half tube, the other is in front. The two opposite positions that tube can be brought to are in order that the instrument may be placed either side of a window, and each of those positions is determined by one end of a cut at the bottom of the tube in *a, fig. 3.* which then holds against the steady pin *c, fig. 4.* The rows of holes of the tube are to be turned towards the room, to prevent the rain from falling on the *slip*; and the *dial* being inclosed in a box with a glass in front, no rain can get into the instrument. It must also be fixed in a place not much exposed to the sun, or be screened from it without preventing the circulation of the air. The communication of the *slip* with the external air through the rows of holes and the open bottom *d, fig. 4.* is sufficient for that class of observations. By the manner of its being hooked, it may be easily taken off for carrying elsewhere; and, if a quick observation is wanted, the tube also may easily be taken off.

I have the honour of presenting one of those instruments to the Royal Society; and, as it is very desirable that some hygrometer be added to the other meteorological instruments usually observed, I wish this may deserve a place in their Observatory for that purpose.



Fig. 1.

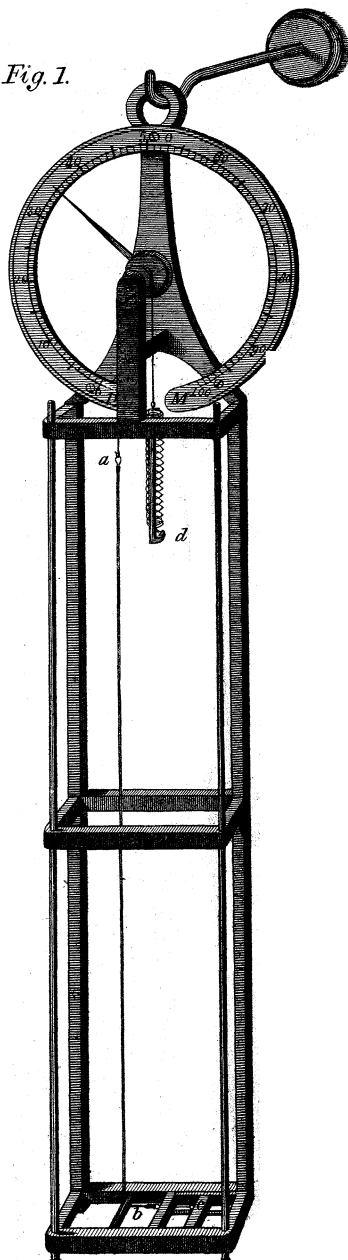


Fig. 2.

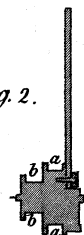


Fig. 3.

Fig. 4.

